The Contextual Theory of Explanation and Inference to the Best Explanation

Seungbae Park
Ulsan National Institute of Science and Technology
The Republic of Korea

Abstract

Van Fraassen explains rejections and asymmetries in science in terms of his contextual theory of explanation in the same way that scientists explain observable phenomena in the world in terms of scientific theories. I object that van Fraassen's skeptical view regarding inference to the best explanation together with the English view of rationality jointly imply that the contextual theory is not rationally compelling, so van Fraassen and his epistemic colleagues can rationally disbelieve it. Prasetya replies that the truth of the contextual theory coincides with its empirical adequacy, so the contextual theory is compelling. He also replies that the contextual theory is compelling, provided that van Fraassen's argument for it instantiates a compelling subset of IBE. I argue that Prasetya's replies either fail or help scientific realism.

Keywords

Argument from a Bad Lot, Contextual Theory, Inference to the Best Explanation, Scientific Realism

1. Introduction

Suppose that a hypothesis, H, better explains phenomena than do its rival hypotheses. We use inference to the best explanation (IBE) when we infer the truth of H from the best explanation of phenomena. IBE is used not only in science but also in philosophy, as many philosophers (Laudan, 1981, p. 19; Glymour, 1984, p. 173; van Fraassen, 1989, p. 13; Okasha, 2000, pp. 691–692) observe. An interesting issue arises as to whether IBE is rationally compelling, i.e., whether rational agents are required to believe that H is true.

Bas van Fraassen (1989, p. 143) constructs the argument from a bad lot to argue that IBE is not compelling. It holds that H might have unconceived rivals, so H needs to be demonstrated to be better than its unconceived rivals. Van Fraassen states, "I watch no contest of the theories we have so painfully struggled to formulate, with those no one has proposed" (van Fraassen, 1989, p. 143). He merely points out the possibility that there might be unconceived rivals. He does not claim that such rivals are likely to exist. Consequently, he does not bear the burden of presenting evidence to show that there are unconceived rivals.

In addition, van Fraassen adopts the English view of rationality, according to which "what it is rational to believe includes anything that one is not rationally compelled to disbelieve" (van Fraassen, 1989, pp. 171–172). On this account, it is rational to disbelieve H unless we are compelled to believe H, and it is rational to believe H unless we are compelled to disbelieve H. Van Fraassen asserts that we are not compelled to believe H, so it is rational to disbelieve H. Scientific realists might protest that H best explains phenomena, so we are compelled to believe H. Van Fraassen replies that although H best explains phenomena, we are not compelled to believe it because, among other things, it might have unconceived rivals. He also claims that we are not compelled to disbelieve H, so it is rational to believe H. Scientific pessimists might protest that we are compelled to disbelieve H due to the pessimistic induction. Van Fraassen does not have a story of how we can rationally believe H despite the pessimistic

induction (Park, 2020a, p. 5).

Van Fraassen (1980, Chapter 5) develops and justifies the contextual theory of explanation (CT). The following questions arise: What method does he use to establish the CT? Does he use IBE or not? If he does, the CT would not be compelling, so we could rationally disbelieve it. Does the truth of the CT coincide with its empirical adequacy? If it does, the CT might be compelling, so it might be irrational to disbelieve the CT. I (2017a, 2019a, 2019b, 2020b) and Yunus Prasetya (2021) offer opposing answers to these questions. I criticize van Fraassen's position, whereas Prasetya defends it against my criticisms. In this paper, I defend my previous position and criticize Prasetya's position.

I organize the present paper as follows. In Section 2, I analyze van Fraassen's (1980) argument for the CT to show that he indeed uses IBE to justify the CT. In Section 3, I argue that van Fraassen's critics can rationally disbelieve the CT. In Section 4, I criticize Prasetya's view that the truth of CT is identical with its empirical adequacy. In Section 5, I turn the argument from a bad lot against Prasetya's suggestion that the CT is compelling, provided that van Fraassen's argument for it instantiates a compelling subset of IBE. In Section 6, I explore how scientific realists can make use of Prasetya's insights to defend scientific realism against scientific antirealists' critiques. In Section 7, I argue Prasetya's defense is better than other philosophers' standard defense of van Fraassen's position.

The main thesis of this paper is that van Fraassen uses IBE to justify the CT, so it is rational for all of us, including van Fraassen, to disbelieve the CT. In case Prasetya's original defense of van Fraassen's position succeeds, his insights can be used to support scientific realism; in case it fails, we can rationally disbelieve the CT, as I claimed. Hence, this paper can be useful to those who aim to investigate whether scientific realism can be defended in the way scientific antirealists' positive theory can be defended, as well as to those who aim to investigate whether scientific antirealists' positive philosophical theory can survive their own criticisms against scientific realism.

2. Van Fraassen's Argument for the CT

Van Fraassen's justification of the CT parallels scientists' justifications of scientific theories. In addition, van Fraassen's own description of what he does to establish the CT parallels philosophers' descriptions of what scientists do to establish scientific theories. I explicate these two similarities in detail in this section. Keep the similarities in mind, for they will be invoked in the subsequent sections where I critically respond to Prasetya's defense of van Fraassen's position.

According to Newton's theory of gravity, gravity exists between material objects. Suppose, for example, that scientists observe that a tree falls down, that a pendulum moves left and right, and that a planet revolves around the sun. They describe these phenomena without using the theoretical term "gravity" that figures in the theory of gravity. After describing the phenomena, they explain the phenomena in terms of the theory of gravity. When they do so, they use the theoretical term "gravity," saying that gravity underlies the motions of a tree, a pendulum, and a planet. This explanation illuminates the motions of the material objects, and we gain insight into them. Keep in mind that the theoretical term "gravity" does not figure in the descriptions of the phenomena, but it does figure in the theory of gravity and in the explanation of the phenomena in terms of the theory of gravity.

According to the CT, "An explanation is an answer to a why-question" (van Fraassen, 1980, p. 134), and "among the scientifically relevant factors, context determines explanatorily relevant ones" (van Fraassen, 1980, p. 126). Terms including "context," "factors," "relevant," and "why-question" figure in this brief formulation of the CT. Let me call those and related terms "contextual terms" from now on. Contextual terms in the CT are the correlates of

theoretical terms in scientific theories, as will soon become clear.

Van Fraassen (1980, pp. 111–112) provides several examples of rejections and asymmetries that a theory of scientific explanation should account for. When he describes them, he does not use contextual terms. Let me introduce only one example of a rejection and one example of an asymmetry to save space. A rejection occurs when a request for an explanation is rejected. For example, suppose that Jones and Smith got syphilis, whereas Tom did not. Jones suffers from paresis, but Smith and Tom do not. Why is it that Jones, but not Tom, suffers from paresis? Medical science accepts this request for an explanation. Why is it that Jones, but not Smith, suffers from paresis? Medical science rejects this request for an explanation. Let me move on to an example of an asymmetry. An asymmetry occurs when X can explain Y, but not vice versa, although X entails Y and vice versa. Consider the famous flagpole objection to Carl Hempel's (1966) deductive-nomological model of explanation. We can explain why the shadow of a flagpole is of a certain length by appealing to the length of the flagpole, but not vice versa (van Fraassen, 1980, p. 112).

After describing rejections and asymmetries without using contextual terms, van Fraassen proposes that a successful and correct theory of scientific explanation should accommodate and account for asymmetries and rejections:

> To be successful, a theory of explanation must accommodate, and account for, both rejections and asymmetries. I shall now examine some attempts to come to terms with these, and gather from them the clues to the correct account. (van Fraassen, 1980, p. 112)

After putting forward this proposal, van Fraassen (1980, p. 128) applies the CT to the example of paresis with the use of contextual terms. He says that the request for an explanation of why Jones, but not Tom, suffers from paresis is accepted because the contrast-class is Tom and because medical science has the explanation that Jones suffers from paresis while Tom does not because Jones got syphilis while Tom did not. By contrast, the request for an explanation of why Jones, but not Smith, suffers from paresis is rejected because the contrast-class is Smith and because medical scientists have no explanation of why Jones suffers from paresis while Smith does not.

After explaining rejections in terms of the CT, van Fraassen predicts that the CT can also explain asymmetries, which he claims will be a crucial test for the CT and against its rivals (Hempel, 1966; Salmon, 1971; Friedman, 1974):

> That vexing problem about paresis, where we seem both to have and not to have an explanation, was solved by reflection on the contextually supplied contrast-class. That equally vexing, and much older, problem of the asymmetries of explanation, is illuminated by reflection on the other main contextual factor: contextual relevance. (van Fraassen, 1980, p. 130)

> In addition, it should then also be possible to account for specific asymmetries in terms of the interests of questioner and audience that determine this relevance. These considerations provide a crucial test for the account of explanation which I propose. (van Fraassen, 1980, pp. 130–131).

After making this prediction, van Fraassen claims that "asymmetries must be at least sometimes reversible through a change in context" (van Fraassen, 1980, p. 132). To flesh out this claim, he (1980, pp. 132–134) generates a context in which it is acceptable to explain why a tower is of a certain height by appealing to the length of its shadow.

So far, I have sketched how van Fraassen justifies the CT. I take note of the following three important things about how he justifies the CT: (1) He first describes several examples of rejections and asymmetries without using contextual terms. What he does in this context

parallels what scientists can do with respect to the theory of gravity. Recall that scientists can first describe the motions of a tree, a pendulum, and a planet without using the theoretical term "gravity."

(2) Just as scientists use the theoretical term "gravity" to explain the motions of a tree, a pendulum, and a planet in terms of the theory of gravity, so van Fraassen uses contextual terms to explain rejections and asymmetries in terms of the CT. I ask readers to take a close look at the two consecutive quoted passages right above. The two passages include the contextual terms "contextually," "contrast-class," "contextual factor," "contextual relevance," "interests of questioner and audience," and "relevance." Such contextual terms are found in van Fraassen's (1980, pp. 128–132) explanations of rejections and asymmetries, but not in his (1980, pp. 111–112) descriptions of the several examples of rejections and asymmetries.

(3) Just as philosophers of science use philosophical terms such as "problems," "successful," "correct," "accommodate," "crucial test," and "illuminate" to describe what scientific theories do, so van Fraassen uses those philosophical terms to describe what the CT does. For example, philosophers of science say that scientific theories solve problems and illuminate phenomena; likewise, van Fraassen says that the CT solves the problems of rejections and asymmetries and illuminates rejections and asymmetries. This indicates that he uses IBE to justify the CT, just as scientists use IBE to justify their hypotheses.

Let me provide further textual evidence to show that van Fraassen's argument for the CT uses IBE. In multiple places, van Fraassen (1980) states that a scientific theory *accounts for* phenomena. For example, he says that scientific "theories account for the phenomena (which means, the observable processes and structures) by postulating other processes and structures not directly accessible to observation" (van Fraassen, 1980, p. 3). Moreover, he claims that the CT "explains the tension we feel in the paresis example" (van Fraassen, 1980, p. 128). Note that van Fraassen uses "account for" and "explain" interchangeably. For him, to say that the CT accounts for rejections and asymmetries means that the CT explains rejections and asymmetries.

In addition, van Fraassen observes that IBE is widely used in human enterprises, including philosophy, saying that "The inference from the phenomena that puzzle us, to their best explanation, appears to have our instinctive assent. We see putative examples of it, in science and philosophy no less than in ordinary life and in literature" (van Fraassen, 1989, p. 131). This observation of IBE agrees with the observation of other philosophers mentioned in Section 1 above. Consequently, a strong argument is required to claim that van Fraassen does not use IBE to justify the CT.

3. Why Fuss?

Why fuss about the fact that van Fraassen employs IBE to establish the CT? I developed a philosophical position called "epistemic reciprocalism," which asserts that "*ceteris paribus*, we ought to treat our epistemic colleagues in the way they treat their epistemic colleagues" (Park, 2017a, p. 56). Epistemic reciprocalism is in line with the suggestion that there "is no reason for thinking that the Golden Rule ranges over moral matters, but not over epistemic matters" (Park, 2018a: 77–78) and with the epistemic imperative: "*Act only on an epistemic maxim through which you can at the same time will that it should become a universal one*" (Park, 2018b: 441). Van Fraassen disbelieves H although it best explains phenomena, saying that it is not compelling and thus we can rationally disbelieve it. In response, epistemic reciprocalists would disbelieve the CT although it might best explain rejections and asymmetries, saying that it is not compelling and thus we can rationally disbelieve it.

This epistemic disadvantage of van Fraassen's position incurs pragmatic disadvantages. Suppose, for example, that van Fraassen believes that the CT passed the crucial test, so it is a

virtuous theory. As a result, he is confident that it is true and applies for a scholarly award. To his dismay, however, the award committee rejects his application for the reason that it disbelieves the CT. The committee operates under epistemic reciprocalism and under the policy that granting an award requires that it should believe that an applicant's theory is true. Van Fraassen protests that the CT passed the crucial test, so it is compelling. The award committee replies that although it passed the crucial test, it is not compelling because it might compete with unconceived rivals, so they can rationally disbelieve it (Park, 2019a, pp. 92−93).

If van Fraassen believes that H is empirically adequate, epistemic reciprocalists would also believe that the CT is empirically adequate. How does the truth of the CT differ from its empirical adequacy?

> The truth of the theory means that an explanation is an answer to a why-question, and that appropriateness of the answer depends on context. The empirical adequacy of the theory, on the other hand, means that what it explains, viz., the phenomena, such as rejections and asymmetries, occur in scientific practices. (Park, 2017a, p. 61)

Consequently, epistemic reciprocalists believe, for example, that medical science rejects the request to explain why Jones, but not Smith, suffers from paresis, but disbelieve that the request is rejected because the contrast-class is Smith. They also believe that it is acceptable to explain why the tower is of a certain height by appealing to the length of its shadow, but disbelieve that the explanation is acceptable because the interest of the questioner determines a relevant factor.

I must add that the award committee is not compelled to discard the policy that granting an award requires that it should believe that an applicant's theory is true, and thus they can rationally retain it to the disappointment of those who only believe that their theories are empirically adequate (Park, 2019a, p. 96). Thus, believing that H is empirically adequate, but not that it is true, might incur epistemic and pragmatic disadvantages.

4. Truth and Empirical Adequacy

How would van Fraassen reply to my objection sketched in Section 3 above? Prasetya develops an intriguing line of reasoning on behalf of van Fraassen. He argues that van Fraassen may only believe "the contextual theory to be empirically adequate" (Prasetya, 2021, p. 3). However, the truth of the CT coincides with its empirical adequacy, and thus van Fraassen may believe that the CT is true (Prasetya, 2021, p. 4). As a consequence, the alleged epistemic and pragmatic disadvantages of van Fraassen's position evaporate. I critically examine this defense in this section.

Let me first clarify what it means to say that truth is identical with empirical adequacy. Consider the empirical generalization that all flamingos are red. Its truth coincides with its empirical adequacy. After all, to say that it is true means that all flamingos are red, but to say that it is empirically adequate also means that all flamingos are red. Note that the empirical generalization makes a claim about observables, but not about unobservables, and that for this reason its truth does not go beyond its empirical adequacy. Therefore, we can say that if H concerns only observables, its truth is identical with its empirical adequacy.

How does Prasetya attempt to establish that the truth of the CT is the same as its empirical adequacy? He does not say that the CT concerns only observables; rather, he says the following:

> After all, the asking and answering of why-questions seem just as obvious in scientific practice as rejections and asymmetries. If this is true, then belief in the empirical adequacy of the contextual theory involves believing that explanations are answers to why-questions, quite contrary to what Park claims. (Prasetya, 2021, p. 4)

Note that according to Prasetya, what the CT says is just as obvious as what it accounts for, viz., rejections and asymmetries, and thus the truth of the CT coincides with its empirical adequacy.

What are we to make of Prasetya's suggestion? The authors of the alternatives to the CT would also claim that their theories are just as obvious as explanatory phenomena in science, so the truth of their theories coincides with their empirical adequacy.[1] Hempel (1966) would say that the truth of the deductive-nomological model is identical with its empirical adequacy, so it is irrational to disbelieve that to give scientific explanations is to give deductive arguments which invoke laws of nature. Wesley Salmon (1971), Michael Friedman (1974), and even scientists would say the same thing *mutatis mutandis* about their theories. It seems that obviousness is not an adequate criterion for distinguishing between cases in which the truth of philosophical theories coincides with their empirical adequacy and cases in which the truth of philosophical theories goes beyond their empirical adequacy.

Moreover, van Fraassen claims that rejections and asymmetries have been *vexing problems* for theorists of scientific explanation, as the second quoted passage in Section 2 above indicates. If the CT had been as obvious as rejections and asymmetries, as Prasetya claims, it is not clear why other theorists of explanation could not come up with the CT. Nor is it clear why rejections and asymmetries have been problems for theorists of scientific explanation in the first place. In general, a problem is a problem because a solution to it is not obvious. If a solution to a problem is as obvious as the problem, the putative problem would not be a problem in the first place.

How can we show that the truth of the CT goes beyond its empirical adequacy? Consider the theory of gravity. The theoretical term "gravity" figures in it, but not in the descriptions of the motions of a tree, a pendulum, and a planet. It follows that the truth of the theory of gravity goes beyond its empirical adequacy. Similarly, as we noted in Section 2, contextual terms figure in the CT, but not in van Fraassen's descriptions of rejections and asymmetries. It follows that the truth of the CT goes beyond its empirical adequacy. Just as the thesis that the theory of gravity is empirically adequate entails (in conjunction with auxiliaries) that the motions of the material objects exist, but not that gravity exists, so the thesis that the CT is empirically adequate entails that rejections and asymmetries exist, but not that context determines a relevant factor.

Moreover, consider the empirical generalization that all flamingos are red. The empirical generalization is a mere description of observables. With this description, you cannot solve the problem of why a flamingo is red, you cannot unveil the underlying mechanism behind the color of flamingos, and you cannot illuminate the color of flamingos. To say that a flamingo is red because all flamingos are red is not to give an illuminating explanation of why a flamingo is red but rather to replace a mystery with all the mysteries similar to it (Leplin, 1997, p. 23; Park, 2014a, pp. 285–286). As Jarrett Leplin puts it, "Generalizations do not explain their instances" (Leplin, 1997, p. 23).

Consider the theory of gravity. If you believe that it is true, you can believe that it solves the problem of why a tree fell down, that it unveils the underlying mechanism behind the motions of material objects, and that it illuminates the motions of materials objects. By contrast, if you only believe that it is empirically adequate, you cannot believe that the theory of gravity does those things, and you can only believe that a tree falls down, a pendulum moves left and right, and a planet moves around the sun. Accordingly, when asked to explain why a

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[1] Van Fraassen would object that the alternatives cannot account for rejections and asymmetries, so they are not empirically adequate (Prasetya, personal communication). I leave it open whether the approximate truth of the alternatives coincides with their empirical adequacy.

tree falls down, you can only give the unilluminating explanation that it falls down because it falls down, a pendulum moves left and right, and a planet moves around the sun.

Van Fraassen (1980, p. 12) would object that we can invoke the theory of gravity to explain why a tree falls down although we only believe that it is empirically adequate. In my view, however, if you explain phenomena in terms of H despite the fact that you only believe that H is empirically adequate, you run into Moore's paradox (Park, 2017b, p. 383), you face disconcerting questions (Park, 2017b, p. 383), and your speech acts are unethical (Park, 2020c, pp. 182–183). These three problems indicate that belief in the truth of H is required to explain phenomena in terms of H. Unfortunately, it would take us too far afield to explore these problems here.

In any event, if you believe that the CT is true, you can believe that it solves the problems of rejections and asymmetries, unveils the truth behind rejections and asymmetries, and illuminates rejections and asymmetries. By contrast, if you only believe that the CT is empirically adequate, you cannot believe that the CT does those things, and you can only believe that rejections and asymmetries exist in science. Accordingly, when asked to explain an instance of a rejection, you can only provide all instances of rejections and asymmetries as explanantia, thereby giving an unilluminating explanation of the instance. Again, to explain an instance in terms of all instances similar to it is merely to replace a mystery with all mysteries similar to it.

Relatedly, the theory of gravity unifies the motions of a tree, a pendulum, and a planet. It tells us that although the motions of the material objects appear to be disparate phenomena, they all occur due to gravity, and thus they are the same kind of phenomenon. However, if you only believe that the theory of gravity is empirically adequate, you cannot believe that they all occur due to gravity, and you can only believe that they are disparate phenomena. To put it differently, you cannot have the unified picture of the world that results from the theory of gravity. It is to try to have one's cake and eat it at the same time to only believe that the theory of gravity is empirically adequate and to also believe that the motions are the same kind of phenomena. In other words, the unified picture is not available for those who only believe that the theory of gravity is empirically adequate.

Analogously, the CT unifies rejections and asymmetries. The CT implies that although rejections and asymmetries appear to be disparate phenomena, they all occur due to the context-dependence of a relevant factor, and thus they are the same kind of phenomena. However, if you only believe that the CT is empirically adequate, you cannot believe that they all occur due to the context-dependence of a relevant factor, and you can only believe that they are disparate phenomena. In other words, you cannot have the unified picture of science that results from the CT. It is to try to have it both ways to only believe that the CT is empirically adequate and to also believe that rejections and asymmetries are the same kind of phenomena. In other words, the unified picture is not available for those who only believe that the CT is empirically adequate.

In sum, just as to believe that the theory of gravity is empirically adequate is to believe far less than what the theory of gravity says about the world, so to believe that the CT is empirically adequate is to believe far less than what the CT says about science, *pace* Prasetya. The other side of the coin is that just as if you aim to fully capture what the theory of gravity says about the world, you should believe that it is true and that its truth goes beyond its empirical adequacy, so if you aim to fully capture what the CT says about science, you should believe that it is true and that its truth goes beyond its empirical adequacy.

This conclusion has a noteworthy implication. It is a double standard to believe that H is empirically adequate but to believe that the CT is true, or to disbelieve H but to believe the CT. Van Fraassen disbelieves H on the grounds that H is not compelling, so it is rational to

disbelieve H. He speaks, however, as if the CT were true. He says, for example, "among the scientifically relevant factors, context determines explanatorily relevant ones" (van Fraassen, 1980, p. 126). In response, scientists who embrace epistemic reciprocalism would turn the double standard against van Fraassen, i.e., they would believe their theories but disbelieve the CT (Park, 2019a, p. 91).

5. A Compelling Subset of IBE
Prasetya stakes out another original path to defuse my objection that since van Fraassen's argument for the CT instantiates IBE, the CT is not compelling. According to Prasetya, "even if van Fraassen's argument for the contextual theory instantiates IBE, he can claim that it is compelling" (Prasetya, 2021, p. 7). I critically respond to Prasetya's argument for this claim in this section.

Prasetya (2021, p. 7, footnote) calls "inference to any statement" the rule of inference in which any conclusion is drawn from a premise. It appears that all arguments instantiating inference to any statement are uncompelling. On close examination, however, some arguments instantiating it are compelling while other arguments instantiating it are uncompelling. For example, an argument instantiating it would be compelling provided that it instantiates *modus ponens*, which is one of the many rules of inference that are all subsumed under inference to any statement (Prasetya, 2021, p. 7, footnote).

By parity of reasoning, Prasetya continues, van Fraassen's argument for the CT instantiating IBE would be compelling provided that it instantiates a compelling rule of inference, which is one of the rules of inference which are all subsumed under IBE. An example of such a rule of inference might be inference to the only adequate explanation (IOAE). IOAE is a rule of inference in which the best explanation is inferred on the grounds that it is adequate but its conceived rivals are all inadequate. So what?

> Merely demonstrating that van Fraassen's argument for the contextual theory instantiates IBE is not enough. Park must also show that van Fraassen's argument for the contextual theory does not instantiate IOAE or some other safe argument form. (Prasetya, 2021, pp. 7–8)

Prasetya does not claim that IOAE is compelling, nor does he claim that van Fraassen would take it to be compelling. He merely uses it as an example of a subset of IBE that might be compelling. Therefore, his defense of van Fraassen's position against my objection comes down to the request that I should show that van Fraassen's argument for the CT does not instantiate a compelling subset of IBE, which might be or might not include IOAE.

Speaking of IOAE, other proponents of van Fraassen's position might be tempted to argue that van Fraassen's argument for the CT instantiates IOAE, so the CT is compelling. In my view, however, resisting this temptation is better than yielding to it because it is problematic to say that the CT is adequate while its alternatives are inadequate. It is not clear whether the CT can explain asymmetries, although van Fraassen claims that it does. As Edwin Hung (1997, p. 179) observes, the CT cannot handle the example of the tower. What really explains the height of the tower is not the length of its shadow but rather the motive of the tower-builder. Therefore, it is questionable whether there is a context in which the length of an object can be legitimately explained in terms of the length of its shadow. Moreover, although van Fraassen (1980, p. 112) claims that the deductive-nomological model cannot handle the flagpole example, Hempel can say, in my view, that the shadow of the flagpole cannot be an initial condition for the flagpole, so the flagpole example does not fit the deductive-nomological model in the first place, and it cannot be a counterexample to the deductive-nomological model. In addition, the CT has a few other problems (Salmon, 1989, pp. 135–150). For example, some

scientific explanations are not answers to why-questions but rather answers to how-questions. All these objections to the CT make it doubtful that the CT is adequate while its alternatives are inadequate.

Let me make another critical comment regarding IOAE. Van Fraassen's attacks on IBE apply no less to IOAE. For example, van Fraassen's (1989, p. 143) argument from a bad lot holds that H is not compelling, although it best explains phenomena, because it might have unconceived rivals. He states, "I watch no contest of the theories we have so painfully struggled to formulate, with those no one has proposed" (van Fraassen, 1989, p. 143). In my view, we do not watch the contest between H and its unconceived rivals, whether its conceived rivals are adequate or inadequate. It is false that we watch the contest when its conceived rivals are inadequate, but that we do not when its conceived rivals are adequate. Therefore, if the argument from a bad lot makes IBE uncompelling, it also makes IOAE uncompelling.[2]

Now, how do I respond to Prasetya's argument that van Fraassen's argument for the CT might instantiate a compelling subset of IBE, so I should show that van Fraassen's argument for the CT does not instantiate such a subset of IBE? It does not matter whether van Fraassen's argument for the CT instantiates IBE or not. What matters is whether van Fraassen's attacks on H also apply to the CT.[3] I (2017c, p. 27) argue that the argument from a bad lot applies no less to the CT. We do not watch the contest between the CT and its unconceived rivals any more than we watch the contest between H and its unconceived rivals. Therefore, if the argument from a bad lot makes H uncompelling, it also makes the CT uncompelling. Once I jettison my claim that van Fraassen uses IBE to justify the CT, I do not have the burden of meeting Prasetya's request that I should show that van Fraassen's argument for the CT does not instantiate a compelling subset of IBE. Yet, my point that the CT is uncompelling by van Fraassen's own light remains unscathed, so van Fraassen and his epistemic colleagues can rationally disbelieve it.

6. Scientific Realism
So far, I have attempted to rebut Prasetya's defense of van Fraassen's position. However, it does not matter whether my attempt succeeds or fails. If it succeeds, van Fraassen's position has epistemic and pragmatic disadvantages, as I (2017a, 2019a, 2019b, 2020b) claimed. In other words, we can rationally disbelieve the CT, and this epistemic disadvantage of van Fraassen's position comes with pragmatic disadvantages. If my attempt fails, scientific realists can utilize Prasetya's insights to defend scientific realism against scientific antirealists' objections. Let me sketch how scientific realists can utilize Prasetya's insights in this section.

Scientific realism holds roughly that successful theories are true. Putnam (1975, p. 73) uses IBE to establish it in his famous "the no-miracles argument," which holds roughly that scientific realism best explains why science is a successful enterprise, so we can justifiably believe that scientific realism is true. Scientific antirealists object that the no-miracles argument instantiates IBE, so it begs the question against skeptics of IBE. For example, Larry Laudan says that it is "a monumental case of begging the question" (Laudan, 1981, p. 45). Analogously, Arthur Fine says that it is "a paradigm case of begging the question" (Fine, 1991, p. 82).

Appealing to Prasetya's insight, scientific realists can argue that just because the no-miracles argument instantiates IBE, it does not follow that it is uncompelling, and that it begs the question against skeptics of IBE. Before saying that it begs the question against skeptics of IBE, Fine and Laudan must show that the no-miracles argument does not instantiate a compelling subset of IBE. After all, if it instantiates such a subset, scientific realism would be

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[2] I do not mean to suggest, however, that the argument from a bad lot is flawless. See Park (2020d, pp. 62–68, 2021) for a recent discussion of the argument from a bad lot.
[3] I thank Prasetya (personal communication) for this point.

compelling, and thus it would be irrational for scientific antirealists to disbelieve it.

Suppose that IOAE is such a subset of IBE. Scientific realists can argue that the no-miracles argument instantiates IOAE. The realist explanation of why science is a successful enterprise competes with its antirealist alternatives. There are nine such antirealist alternatives in the literature. All of them make different assertions about why science is a successful enterprise. I (2014b) expose flaws with eight of them and then runs a pessimistic induction against the ninth and its future successors. If this pessimistic induction is correct, the prospect is gloomy that scientific antirealists will arrive at a viable explanation of why science is a successful enterprise, and the realist explanation will remain as the only adequate explanation. Consequently, scientific realism is compelling, although it is a product of IBE.

Suppose also that Prasetya is right that regardless of whether IOAE is compelling or not, I should show that van Fraassen's argument for the CT does not instantiate a compelling subset of IBE before he concludes that CT is not compelling. Scientific realists would say the same thing *mutatis mutandis* against van Fraassen, who claims that H is not compelling. Just because scientists' argument for H instantiates IBE, it does not follow that H is not compelling. After all, scientists' argument for H might instantiate a compelling subset of IBE. Before concluding that H is not compelling, van Fraassen must show that scientists' argument for H does not instantiate a compelling subset of IBE. This challenge to van Fraassen echoes van Fraassen's challenge to scientific realists that they must show that H might have unconceived rivals, so they must show that H is better than its unconceived rivals.

In sum, scientific realists can utilize Prasetya's insights in case my attempt to rebut Prasetya's defense of van Fraassen's position fails. The only way for proponents of van Fraassen's position to avoid this consequence is to show that there are relevant differences between Prasetya's defense and scientific realists' defense. They would have to indicate the relevant differences between the no-miracles argument for scientific realism and van Fraassen's argument for the CT. The relevant differences should demonstrate that the no-miracles argument is not compelling while van Fraassen's argument is compelling. In addition, they would also have to indicate relevant differences between Prasetya's argument against me and scientific realists' argument against van Fraassen. Prasetya' argument against me is that van Fraassen's argument for the CT might instantiate a compelling subset of IBE, so I should show that van Fraassen's argument for the CT does not instantiate a compelling subset of IBE. Scientific realists' argument against van Fraassen is that scientists' argument for H might instantiate a compelling subset of IBE, so van Fraassen must show that scientists' argument for H does not instantiate a compelling subset of IBE. It is beyond my imagination how defenders of van Fraassen's position could accomplish these tasks.

7. Prasetya's Defense vs. the Standard Defense

Van Fraassen has been an influential philosopher in the scientific realism debate. His position has been a focus of the debate between many rival philosophers of science. His opponents criticize it, while his proponents defend it against the opponents' criticisms. In this tradition, I criticize it, while Prasetya defends it against my criticisms. However, Prasetya's defense is unprecedented and profoundly different from other philosophers' standard defense. In this section, I argue that Prasetya's defense is better than the classic defense.

How do the two defenses differ from each other? The classic defense has been to say to van Fraassen's critics, "Van Fraassen doesn't believe what you think he does, so your criticism misfires!" By contrast, Prasetya's defense is to say to me, "Van Fraassen believes what you think he doesn't, so your criticism misfires!" Let me flesh this abstract comparison with the following two examples.

(a) Consider first the debate between Stathis Psillos and van Fraassen's proponents.

Psillos (1996, pp. 33−34) attributes to van Fraassen the position that IBE is compelling when H concerns only observables, i.e., when the truth of H coincides with its empirical adequacy. James Ladyman et al. retort that according to van Fraassen, H is not compelling even if it concerns only observables,[4] i.e., "IBE is unacceptable in general" (Ladyman et al., 1997, p. 312). (b) Psillos (1997, p. 370) observes that epistemic risk is involved in the inductive inference that since H is successful, it is empirically adequate. Brad Wray retorts that van Fraassen "is not committed to claiming that our best theories are in fact empirically adequate" (Wray, 2012, p. 378). In short, it has been the standard move for proponents of van Fraassen's position to say that he does not believe what critics think that he does (Park, 2019b, p. 153).

Compare the standard defense with Prasetya's defense against my criticisms. I claim that van Fraassen uses IBE to justify the CT, so it is not compelling, and it is rational for him and his epistemic colleagues to disbelieve it, and this epistemic disadvantage incurs pragmatic disadvantages. Prasetya replies that the truth of the CT is the same as its empirical adequacy, so the truth of the CT is compelling, which implies that van Fraassen and his epistemic colleagues are compelled to believe the CT, and that it is irrational for them to disbelieve the CT.

As far as I am concerned, Prasetya's defense is better than the standard defense. The standard defense attributes skepticism to van Fraassen, and skepticism comes with epistemic and pragmatic disadvantages, as the example of the award committee in Section 3 illustrates. If van Fraassen does not even believe that H is empirically adequate, epistemic reciprocalists would not even believe that CT is empirically adequate. For these reasons, I claimed earlier that the standard defense only brings a Pyrrhic victory to van Fraassen (Park, 2019b, p. 153). By contrast, Prasetya's defense implies that we should believe the CT, thereby providing van Fraassen with the means to avoid the epistemic and pragmatic disadvantages.

Moreover, Prasetya's defense might prove to be the starting point of a new debate between scientific realists and antirealists. I have already presented some challenges for prospective proponents of van Fraassen's position in Section 6 above. These days, scientific realists and antirealists are in a stalemate. Scientific realists have settled for selective realism in light of the pessimistic induction. As Kyle Stanford (2015, p. 875) observes, however, selective realists and pessimists commonly believe that our best theories will be overturned, and thus there is only a terminological dispute between them regarding whether it is justifiable to attribute "approximately true" to our best theories.

I earlier stated that "It is a perennial issue what van Fraassen's position is" (Park, 2020b, p. 38). It is not clear whether it is a virtue or a vice of van Fraassen's position that it admits of opposing interpretations. On the one hand, an elusive position is counter to analytic philosophy, which cherishes clarity more than any other virtues (Park, 2020e, pp. 492–493). On the other hand, the opposing interpretations cast light on hitherto unexplored positions. What matters is not which position van Fraassen holds but rather whether the newly explored positions are advantageous or advantageous from both epistemic and pragmatic points of view. Therefore, it is a remarkable feat for Prasetya to explore the hitherto unexplored paths for those who are sympathetic to the CT.

8. Conclusion

Prasetya's key claims are as follows. (i) The truth of the CT coincides with its empirical adequacy, and thus it is irrational to disbelieve the CT. (ii) The CT is compelling provided that van Fraassen's argument for it instantiates a compelling subset of IBE. I attempted to refute

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[4] If Ladyman et al.'s interpretation of van Fraassen's position is correct, van Fraassen cannot avail himself of Prasetya's defense that the truth of the CT is identical with its empirical adequacy, so its truth is compelling. This reinforces my conviction that retreating to skepticism comes with epistemic and pragmatic disadvantages.

these claims as follows. (a) Contextual terms figure in the CT, but not in the descriptions of rejections and asymmetries, and thus the truth of the CT goes beyond its empirical adequacy. (b) The CT is vulnerable to the argument from a bad lot. Therefore, van Fraassen can rationally disbelieve the CT by his own lights.

At the end of the day, however, it does not matter whether my critical response to Prasetya's defense succeeds or fails. If it succeeds, we can rationally disbelieve the CT. If it fails, scientific realists can utilize Prasetya's ideas to defend scientific realism against scientific antirealists' critiques. In any event, I predict that a further criticism against van Fraassen's positive theory and a further defense of his positive theory will produce a further clue regarding how to defend scientific realism against scientific antirealists' critiques, and that the further clue will prove to be the starting point of a new debate between scientific realists and antirealists. The opposing interlocutors will offer opposing answers to the following question: Can skeptics of science have a positive theory of science?

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